

RKM: I never like to misquote anyone.

JL It had something to do with self-defeating revelations. I said ^{that} like most revelations what I am about to say is simply a restatement of what we already know and probably have said so many times we've forgotten it. Going over my old correspondence is really a traumatic experience. It looks into, you know, very half-remembered material I've totally forgotten, wouldn't have believed if I'd been told about it and that sort of thing. Well, to get to the point, what's this paper all about? [≡] what can it be used for as a case study? We talked last time we were all together about a check list of the use norms of the Mertonian characterizations of science and so on and I very much wanted that. My only question was, is this a particularly good instance to go into it or for the historiographic reasons and problems that we've gone into before ^{it may not be.} ~~and~~ I think needs to be done as well as can be done, ^{so} ~~we~~ we'll see. But there's one theme that we can use this for in an exemplary way ⁻⁻ not just the discovery of recombination but the whole arena of molecular genetics of which it was an important input, and that is the Kuhnian doctrine, that is, to what extent can you even think about a scientific revolution having happened? Has there really been one in biology? Does the common phrase "the revolution in biological science" really

connote what Kuhn was trying to say and in my view does not. I don't see any sudden change of paradigm operating in this field at all. And yet you could have another such talk about another revolution in science having taken place.

And I think working with _____ period models you can do better than I in trying to outline the level of discourse I am referring to here. I think this is an ideal setting for that kind of discussion and for which all of the preliminary detailed remarks and the personal history and intellectual history and so forth are all highly relevant. And so this was my revelation that this is really what we've been groping for and talk about discontinuity in the microscopic sense of an eyesighted discovery captures only a very small part of that. To what extent has there really been discontinuity in the entire development of the field? ^{It has} ~~and~~ in some ways ^{been} ~~very~~ substantial, but I don't think it follows the Kuhnian paradigm at all.

RKM What would it be?

JL I am not sure I totally understand that at all.

RKM One way of saying what I think I heard is that the paper ~~that~~ ^{bearing} ~~appears~~ could provide the exemplar of scientific development on the question of the varieties of revolutions in science. Because I think if we make it, it doesn't fit the pattern period. I don't mean to downgrade that. There are

now a ~~space~~ of such papers in various fields and this might be mistakenly perceived as just another one. There are some in economics, and psychology. If they haven't come across your desk-- ^{we can supply them} The exemplifications are not of the same kind, that's precisely my point, but that if it were ^{treated in this way--} ~~an effect~~, the papers ^{is} ~~is~~ the Kuhnian conception, adequate or not, it will be placed in that class of papers.

JL I understand what you're saying. I don't think we want to be confused with ~~pro Kuhn~~ ^{anti Kuhn} criticisms.

RKM Now we could spend the rest of our time together here by Harriet and I telling you about the conference at Berkeley where you were present on the list but wisely stayed away.

Then someone is looking after you. But now having said that I am now going to apparently report the opposite, that is, in valuable and unexpected ways that had nothing to do or almost nothing to do with the central purpose of the Conference on Quantification and the History of Science. David Edge, it turned out, wrongly emphasizing ^{ed} ~~ing~~ when he took the floor to present his paper, that he was really a radio astronomer and not a mere historian or heaven forbid a mere sociologist. ^{He said that} ~~Thought~~ what they're trying to do is history and sociology. Indeed that their effort is concerned with an historical sociology of science and technology. Now I have to take about five minutes or so to give you the

context in order for me to draw implications for us.

Before that paper was presented, David Edge, Roy McLeod, Arnold Thackeray, Bernie Barber and I had lunch - a business lunch. Edge, and he really was on edge, and McLeod were very concerned because they had heard that the 4S Council a few days ago, just before the conference, decided that it was a reasonable possibility that the 4 S could establish a journal within a year and a half or two. They were distressed and they distorted that into our having made the decision, I let them talk and so on and finally informed them that the decision had been made to look into a journal, ^{their journal Social Studies} which is Suena pretty shaky ground financially as is, and which has been taken over by the Sage publications as a proprietary publication

Well the upshot was that I set them straight, that the Council had decided to look into it, but that they were now being asked what they could do for us. What interest they had in becoming our official journal? There was a lot of jockeying on their part, ^{and expressions} of no interest. They were opposed in principle to organizations of scientists. They felt individuals ought to trust one another and that there ought to be no organizations and that consequently the organization known as the 4 S should allow its members to become subscribers and everything else remain unchanged. The internal contradictions don't have to be spelled out. They're so manifest that they're

hard to take in. Well that didn't get very far after they were informed that there were such things known as ^{professional} ~~honor~~ societies that had been going on for sometime. That each one tended to have its own publications and that by "its own" meant a structural relationship. Namely that the society took responsibility and control of the choice of editors and so on and that this was for them to consider. It's very simple. What they want is for us to declare them the official journal so that it comes with the dues, so they pick up some overlap between the membership let's say, 250 new members. Their current subscription is 800 including institutional.

HZ But they're not going to get rich on it because Sage is the owner.

RKM Well, this is - if they try to get rich Sage is going to drop them.

HZ That's exactly it.

RKM Well, I mean it's not a case of getting rich, it's vital ^{subscribers} for them to get more. Well, I give you all this context quickly And Edge, whom I was meeting for the first time, is quite an impressive looking character, 6 feet two, three, straight kind of English type, with all the kind of eloquence, under-cutting, nasty, all of the stereotype come to life, which

surprised me because Harriet and I had kept in touch with his work and liked some of it /and so on and had gotten the impression he was removed from all this. He gave me a key there, when he was talking, he said two things, "I have to tell you I'm going to be speaking very bluntly after my paper this afternoon and I hope I do not offend you." And I said, "I have no idea what you're going to be saying but obviously you can't offend me because I can't imagine your being offensive." And that took him aback for a moment and then he said, "forgive me if I don't eat very much," no he said, "I won't be eating very much because when I give a paper and particularly this one, I get a nervous stomach." That was all the information I wanted and then he immediately resonated to the memory of Konrad Lorenz, who was one of a small group (about a dozen others) who used to meet 2 or 3 times a year somewhere in Europe. Andre Cournand, Paul Weiss, you know the crowd. And when Konrad had to ~~xx~~ give a presentation or just talk to us, he would be five or ten minutes late invariably and invariably for the same announced reason: I had to go out of doors and vomit and vomit and vomit before I could get ready to talk. And so in a quick diagnosis I decided ~~xx~~ my adversary had delivered himself into my hands for physiological and psychological reasons. And indeed, his was the first paper and he plunged into it. He said now let's bring it all out into the open, there's a great power structure going on in

Struggle

the sociology of science, they are of the entrenched enlightenment types and ^{and there are the romantics and} so on. So all of these six, seven, eight years of backbiting was brought out in ~~the~~ the open. There was obviously a decision ^{about} ~~dedicated~~ making this. Edge had been ~~designated~~ to ~~make~~ the manifesto. And he proceeded to itemize the new vision which their crowd represented. And the new vision was one, to use general sociological theory, nothing trivial. Indeed, he wants to be emphatic, as a radio astronomer, that they're going to use general sociological theory of the mainstream variety, such as Durkheim, and Mannheim and Howard Becker.

HZ And the ethno-methodology tradition.

HB 2km

Well, Howard Becker and the ethno-methodologyists -- That's item one. I won't go through the list I'll give you a précis of it which I'll write up when I get home. But another item was that all this emphasis on quantification was nonsense, still very dubious. A whole set of internal contradictions since later ^(Nigel Gilbert) ~~another~~ claimed that there was very important quantitative work. But that there are certain ~~new~~ books now that give new visions of how knowledge operates, and so on, a book by David Bloor which will appear in a week or two or three, called Knowledge and Social Imagery, which he happened to have the only copy of, and so on, the whole set of items all ~~new~~ against the orthodoxies of enlightenment, and tokenism in science, rationalism in science, no error in science, the straight, stereotyped fable that has been going ^{the} arounds.

After it was over, the meetings were all overscheduled by papers to a very short time so I knew I knew I had no time to speak of but I obviously had to rise just to pinpoint the matter. And the essence of what I said was ["]welcome aboard.["] And I went down point after point after point. Mannheim, well, that's when I came in. My first three papers on the sociology of knowledge were on Mannheim, so if they were discovering reflexivity, they discovered that what you say about others applies to yourself and they don't care if this seems to raise the question of undercutting their own views because they have also discovered or postulated that social causation of ideas does not mean the untruth of those ideas, just

And it was literally beyond belief because the 30 year delay being announced as the new revelation or the new manifesto. So the ~~view~~ welcome aboard was a matter of ^{historical fact} _{him} and as compared - it would be interesting to have/give an account of that same episode, being taped just as this is but without any prior notice that he would be doing that because I had ~~not~~ intended to ^{keep silent} but the tape of what transpired gives you a whole set of self-exemplifying data of perceptions, misperceptions and so on. But the essential point that I want to indicate that in the course of showing that the sociologists also think that nothing matters except aggregated data and so they're all encouraging to (HZ - what ^{Edge} ~~you~~ call clumps) they're number crunchers and they're clumpers and the Mertonian wing which of course

includes the Princan ~~sian~~ variety, are number crunchers and clumpers. And he said, for example, here is a study which will appear soon which is focused on Darwin and Pasteur but focused on just two scientists, this is unthinkable for the sociologists of science of the quantitative variety. But when my ^{turn} ~~term~~ came and I came to that part of the litany, I said, David, I just want to announce, you've managed to discover that there is something called a case study with two people, we've gone you a bit better, we have only one person and that is really what I was really leading up to. That is to say in the cognitive flow of where this field is now, the Kuhn vs. Merton is entering a new phase. They're already making noises that Kuhn turns out to be not all that constructive you'll notice that, the new phase is obviously going to move toward the social construction of reality and a linkage with the ethnomethodologists and that was being announced ^{of} The best indicator/that - in the coffee break I went forward to him - on I left out the most important thing in my diagnosis about the nervous stomach - as I was talking he started to turn white. His head dropped, and for a half hour after I finished my three minutes, he was beaten, a man who was very ill.

HZ

You forgot that he also responded to you Bob. As you can guess Josh, Bob's manner was very amused and jocular. ^{Edge} He responded with a tight, nasty defensive approach and he was obviously

dying on his feet.

Rkm

Well at any rate he was literally, physically crushed. I was startled by this because it was so remote from the presence that I initially saw and then this series of consecutive episodes. So that I went to great pains ~~to~~ in the interval not to make new points but to elaborate the two or three minutes I had. And the group, ten or twelve or fourteen were listening~~x~~ in. The pity was, ~~xxxx~~ several said that it wasn't on tape, that it was the only interesting part of the whole conference. But I won't go into details on the give and take bit and I took the initiative at the ~~ea~~ end of the day, I went to him and it was something of a reconciliation. We had met just that one day so we had gone a complete cycle. Because I was so relaxed then he began to become slightly relaxed.

HZ

I think it's also true that your relaxation had emerged because you had been dreading this confrontation now for ten years.

RKM

Yes, you see this is the biggest surprise. I had a catharsis yesterday which took care of the last 8 years. Because I had been pent up, not writing any rejoinder for 8 years. So those two or three minutes of getting it all out on the record and those two or three minutes in which I could respond and see what the nature of the problem is and the whole thing exemplifying the styles of sociological thought, the polarization, the misperception, the selective reading and so on, so my next substantial

address now which I will have to give in November will be "toward a Historical Sociology of Scientific Knowledge" and I now have the frame. It won't be a rejoinder at all but it will be an analysis which will have the intent at least of supplanting this stereotype. At one point one of the youngsters, Woolgar or Gilbert, I think, they all look like children

around 19 or 20

I could

ordinarily, if they weren't of the English variety, find them very appealing. But they have acquired the academic nonmanners of the English academician which is so frequent there. And one of them said, "Well you couldn't have known all this," - which David Edge had just said. "We can never put any of this in print. How could you have known what our postulates were and our assumptions?" I said, well you see, for the last 30 years or so, several of us engaged in something called "explication ^{de} ~~de~~ ^u texte" which we spend all of our time ~~on~~, There is such a thing, and so on -- So this was a major event for them.

So it is not just a catharsis. In its small way it is a representative moment and thus gives something of a context for what you were suggesting.

JL

I can certainly see that. If I say I was putting my foot on it, that's probably mixing the metaphors.

RKM So that it's all the more- there is all the more reason for us to think of the case study as having a structure, a purpose, a location and so on.

JL I completely agree with the way you formulated it though.

RKM And I will be able to I think, without stretching too much, and without going to other subjects related to the normative structure

JL Have there been any other critical examinations of whether biology has scientific revolutions?

RKM Let me when I get back, pull out everything I have

HZ You must have a lot of stuff and as far as-- my first response is I've seen many times the observation that Kuhn's work may not apply to biology but it may also be true of geology and - or empirical sciences in general.

RKM There are, I would guess, 8, 10, 12 papers which have the question: does the Kuhnian model apply to this seeming case of revolution. I haven't studied those papers but they divide equally or unequally - some say yes and some say no. Now the grounds on which they are saying yes and no what they draw implications with, I can't quote on this. But that was what I was saying earlier. So ~~our~~ obviously one of the things to do if we decide on this and that's the

merit for us. The merit of your proposal because

JL

Well you can ask the same question from a slightly different perspective. I have trouble trying to identify in all modern (?) biology, any event that conforms to that model. I can think of three things that might have been regarded as the most nearly revolutionary in the paradigmatic sense. One of them is mechanism. But that parallel tradition has coexisted in biological thinking controversy about can biology be reduced? It may never terminate.

RKM

As a matter of fact Josh, you may remember my saying that I did the chapter on of the alteration between mechanism and vitalism in Sorokui's Social + Cultural Dynamics I mean I couldn't have been less informed. You don't know about this?

You can imagine, not imagine, I'm telling you that I did 300 pages on this one summer. what I did was read as fast as I could every damn history of biology and so on and it's very thin and so - but the point I want to make in regard to your last statement- if, let's just say that the notion of mechanism or the notion of vitalism were to represent a revolution or a change in any interesting way, then it's a recurrent revolutionary change.

HZ Bob, this brings us back to Gerry Holton's thematic analysis
 and the ^{alienation between} mechanism and vitalism. At any given time,
 positions ^{both} that scientists take on issues.

RKM At any rate so you were saying that's one possible paradigm change.

JL Yes I do think it qualifies, it's not that there are rapid
 oscillations. I think that the conflict is a continuing
 one. Its terms have evolved. They don't mean anything at
 any point as they meant at any previous point. Even, as far
 as I can see there are ^{really} no major discontinuities even in that
 statement although they have evolved Euler

Well his discovery was important but

He may have offended certain people's religious convictions
 but I don't think you can regard that as having brought about
 a paradigm change

RKM Was that a case as formally similar to ^{recombination in} bacteria?

JL Yes, but the question at every time was
 understandable within the framework of the previous paradigms.

RKM And that's your criteria.

JL That's been my criteria.

RKM That's Tom's from the beginning.

- JL I find it very difficult to find revolutions in biology using that criteria.
- RKM Incidentally, you know Marty Klein's paper on Einstein's views on scientific revolutions - I'll get a copy to you - but as I ask these questions I feel very guilty so say naught. Why the hell didn't I, knowing its possible relevance to our subject-- but that's very germane to what you have just put on the record.
- JL Well, the third point I was going to raise was spontaneous generation and its refutation. There again, as important as ^{was} was the demonstration it/ still in a language one could understand, the fundamental ^{there are} and so / revolutionary changes in biology but they're not in the Kuhnian sense
- HZ But they were experienced as revolutionary because of their consequences not because of their origins.
- JL That's correct.
- RKM I pleaded that case I'll now have to go back to it because you were putting it in a totally new context and so you're bringing it alive again. It was dead and you're bringing it back to life. I'm sure that there's

nothing in my supercritical account of quotes "alternations," meaning dominance and so on. But now in terms of these conceptual schemes if the historical count were accurate of oscillations free of dominance then that leads to the notion of the dominant paradigm as distinct from the single paradigm and so on.

And all that stuff, I don't mean my account of it but if I go back to my account of it, it will have some special meaning.

JL But Kuhn was not talking about the competition between and during coexisting paradigms-

RKM No, that's still something else, but he does talk about it now in 1975, he has had to, in the effort to provide a kind of phenomenology of the way science looks and that's still another thing. That's not the one

JL The one case that comes closest is Mendel's. Because the evidence is that others didn't understand. There is a certain prehistory of it but I don't think that disqualifies the idea of it being revolutionary

RKM Could I ask this question? You review these historical cases and see whether ^{they were 'revolutionary'.} It's in a sense looking at them retrospectively, since you know, as it were, their fate and you also have a sense they're well within the boundaries of anything you might want to call science even if you don't define

those boundaries very closely. But the central criterion you were using, and it's Kuhnian, does it fit within the framework, the essential framework, if it does not it will be unintelligible, it will be incommensurable, there can be no communication about the ~~idea~~ ^{conception} and this new conception has to make its way into a hostile environment. But it's more than hostile, it's an uncomprehending environment

But then when you look beyond the boundaries of science, wherever you want to place those boundaries. This falls in the Lakatos-Popper ^{problem of demarcation - the question of} ~~or locuter sense~~ what is science, what is pseudo science, and one or two of the papers of Imre's students who are living off the ^{Lakatos} ~~locuters~~ that ideas - I alluded to / yesterday. The same criterion seems to operate, that is the scientist says - I don't know what the parapsychologist is even asking

HZ No, Bob, I think they understand the question. The question is comprehensible but the evidence is not.

RKM Well let's say a few words on the scientist's handling of astrology in the middle of the 20th century.

JL Well astrology comes closer because there isn't even an effort to adduce the kind of evidence which

RKM Now what is there in the case that is ^{all} ~~be~~ scientific ^{within} along has been granted to be / the scientific even though I don't understand what you said that differentiates ^{para psychology} from astrology.

JL Well So it has a social function. That's the only objective criterion I can offer you. Looking at it as an historian not imposing my own prejudice or wisdom on it.

RKM You don't think there could be cognitive attributes of this unintelligibility that nevertheless make it of a piece with what you are accepting as the doctrine of

JL ? It's ⁱⁿunintelligible but still I can give you a formal analysis of its character. It involves something that looks like experimentation. It involves something that looks like acceptable modes of inference.

RKM Where is the unintelligibility?

JL I don't see it as being a much bigger jump than the notion that determines the for example.

RKM Well where would you come out on the two kinds of unintelligibility? That's where the scientific character is never questioned.

JL

JL Well, in the case of Heisenberg there was, looked at retrospectively, manifest profit in scientists learning that language and beginning to incorporate it into his language and it shattered a lot of illusions about the precision of mechanism. Mechanism hadn't been defined and so forth. But it also made predictions about the outcome of experiments which remain part of the continued tradition. If you ask what is

there that is still the same between pre and post
Heisenbergian physics, the fundamental concept of what
 constitutes an experiment validation and so forth

RKM What keeps it within the scientific tradition when it is
 temporarily regarded as outside your own conceptual frame-
 work in a radical, very radical way. That's what I meant
 by

JL Well you might have to do experiments of a very unusual
 kind. People for a long time/^{would have}regarded transcripts of dreams
 as being totally inappropriate kind of evidence but
 not for reasons that are consistent within the
 scientific framework. There's no fundamental postulate about
 science, the nature of validation and so on that denies any
 aspect of experience as being inappropriate to it. So

RKM Of course I realize

JL Well that's different than using _____ dreams as
 experimental data though. That's the way in which dreams are
 interpreted, the manner in which fears are purportedly demon-
 strated. The clinically retrospective judgement
 is a literary and not a scientific treatment. They're connected
 with that so there are two elements there. That is you have
 a novel kind of experimental material for examination, a thing
 which/^{I think}you know, if you've followed psychology and all the sciences
 we're going to accept, I don't think there is any fundamental

objection to using such reports, but the way they're analyzed is the issue.

RKM

Relating to what you're saying, thinking of this as case of the operating cognitive norms as to what qualifies and what doesn't and so it's the now traditional problem of demarcation between science and nonscience and pseudo science. Popper and Lakatos have made ^{demarcation} a central question and it's apparent to me, that's their big contribution. The notion of ~~falsification~~ ^{falsification} has been around forever and it's been more of a sort of indispensable focusing on an idea that's been around and consequently getting some development of it. But the demarcation issue as part of the problematics has taken a traditional question in all the history of thought. What qualifies as acceptable knowledge in a given culture, and then once you have institutionalized science, what qualifies. But it just may be by returning it to the notion of the norms that operate out there, not the norms imposed by the philosopher of science who says, "I can tell you the difference," but put in the operation of (HZ - the working principle)

JL

Where do we fit history and social science the bulk of it, into this general discussion of what goes on there is amenable

part of analysis, that as a natural scientist, I think we've agreed is the norm for validation and in that sense is history any different from astrology?

RKM Well that's the case then. I think it could be argued that history, in the sense of being concerned with interpreting unique events and patterns or ^aseries of events, sequences, groups of events differs from astrology only in the sense of the normative attitude of organized *Skepticism*. It's up to me, the scholar, to - not to press my case alone but to try to get the kind of evidence that will ~~stand~~ *stand* up to the test.

JL That's begging the question a little, Bob. How is that skepticism manifest? Why would you reject an astrological explanation of a piece of historical fact

RKM No what I meant was that a sophisticated historian will tell you when he will relinquish the belief that the Battle of Hastings occurred in 1066. He will tell you that when such and such documents turn up, that's all I'm saying. He is prepared, and not only he will tell you, but in the way he goes about his business he's continually asking what happened here, is there any way of my finding out?

JL I have a thought about that, the method of history is choose fundamental changes in the physical and the natural sciences.

In fact it ^{relies} ~~relies~~ on the established patterns of explanation of those sciences. That's the fundamental bedrock of what is then a ^{scientific (?)} discipline in a plausible structure. That is it would be very dangerous for historians to attempt to envelope novel science in the course of reconstructing historical explanation. I think it offers the mechanism of the character of that skepticism as long as it doesn't claim too much that is outside of the commonly accepted traditions of how the ~~real~~ world actually runs.

RKM Can you invent an example or an historical explanation or interpretation, that if ipso facto be ruled out as having gone beyond the frame that you've just outlined?

JL Well, contrast history and science fiction. And the style of science fiction which is somewhat self-consciously called

I think the fact that no historian has ever done this to my knowledge, you don't have messengers going faster than the speed of light.

RKM And now the data that constitutes by the historians' response to historical fiction and then there are border line cases where they say this is no more than historical fiction, that is, reconstruction of thoughts that are not recorded anywhere.

Reconstruction of conversations that have no documentary basis, that you move over beyond the pale, and it's not your normative constraint, and it's not only methodological. There can be a lot of methodological conflict. That's not what's at stake. This is beyond the pale. You've broken the norm by introducing notions of factual events, a kind of factual reconstruction without the evidence.

JL Well, I must say historians have to do that all the time. I mean it is impossible to get a complete documentation when there's no way to test by any kind of post some things are plausible All the assertions need to be laid on the line, /you have to put them down and if you're very pedantic I mean I have no in recounting my own biography.

HZ But that's why what is considered illegitimate is a very narrow area ^{covering that} ~~accounting~~ ^{documentary} which has no, because you just can't make it up out of whole cloth.

JL *It might be useful* if we looked at available reviews of either what purports to be history and is critically appraised as being little more than historical fiction or periodically, historical fiction that is reviewed as presenting such distortions even though it's announced fiction, that it threatens the underpinnings of history. Or the third version now of

HZ

RKM

JL

effort in the field. And there is a concerted effort to minimize the extent of that kind of invention. And there are certain norms, especially about things you put in quotation marks~~x~~ that in order to maintain some vestige of objectivity, there are certain rules of presentation that the use of quotes seems to violate. Now I don't see the problem as long as it's made manifestly clear that those quotes are inventions. But that's still a symbolic rendition of - you know if something is in quotes you really had better know ~~xxxx~~ that it was actually said. It's a norm that has its value. I mean see the difficulties that will arise if there are ~~xxx~~ ambiguities about the meaning of an expression.

RKM You said earlier, or I thought you said, that historians would be taking as implicit models what they think of as science. transforming it into what they so that it approximates as close as possible

JL No, I didn't mean that, I meant that the historian's view of the nature of physical reality must not conflict with what the physicist, chemist, and biologist say it is. With the methodological point.

RKM They're trying to aim for it to pattern themselves after scientists

JL They'd better not invent new science.

RKM I see. I didn't understand/the first time. it

JL The historian ^{never} invokes a dragon, or a unicorn even.
It's really

RKM the genuine romantics. These people
are historically speaking the British sociologists of science
as they're acquiring this self image in the course of their
Now the code words are enlightenment
and ~~Tolkienian~~ token, gradualistic types of sociologists -

HZ Mechanistic

RKM Mechanistic, vs. us self-declared romantics. So that how
romantic can you be?, is what Josh is saying - the romantic
historian or romantic sociologist. *They show* a vision of the
way in which nature works.

JL Will David Edge now start reflecting on the basic-

RKM parapsychological
Well, it takes ~~parapsychological~~ processes out there as givens
not as something which should be in an
investigation but as the nature of psychological reality.

JL would
Well, I think that ~~xxx~~ be a very good test case. I can't
think of any historian who would be willing to even hint at
a parapsychological interpretation of some historical event.
He might think that the question of belief in parapsychology
is another issue altogether but-

RKM How romantic can you be is what Josh is saying.

The romantic historian or romantic sociologist, with regard to your vision of the way in which nature works. Will David Edge now start reflecting on the basic - well it takes parapsychological processes as givens not ~~not~~ as something that should be *investigated* but as the nature of psychological reality.

JL Well I think that would be a very good test case. I can't think of any historian who would be will^{ing} to even hint at a parapsychological interpretation of some historical event. The question of belief in parapsychology is another issue altogether. So since he'd be taking on a much larger job within that framework and I don't think historians

RKM Now suppose we were to (JL - Do you know of any exceptions?) No. I think that's a very strong criterion and some of the same observers, let's not call them *romantics* who would say, "Look at the baseless ~~xxxxxxxxxxxx~~ prejudices and attitudes of the allegedly reasonable scientist who will not even look at the evidence for parapsychology, who will not admit it into the family of *sc*iences, whereas I, the observer have an open mind, and I will not accept such a prejudiced notion, ~~xxxxxxx~~ would not use parapsychological interpretations of historical events." Now, when you said earlier that ^{that} scientists--/historians do in fact, according to their own covenants, must accept the world of nature as science has portrayed it. Now we're back to the question, how consistent

is that? Which science?

HZ Well sure, if you look at the efforts in psychological biography for a long time psychoanalyzing in history was held in bad odor, and still is by historians, as being inadmissible, non-scientific, nonhistorical, improper evidence.

JL Also criticized by many psychoanalysts as well -

RKM You see the question I was raising - which world of nature? One answer is that it's not all that different when you get beyond the very deep level and particularly metaphysical.

JL I think they will have their own individual prejudices. They may adopt schools of thought, not necessarily consensual. But I suggest that historians will be doubly sensitive about getting into that realm and will do so only out of sheer ignorance or as a calculated risk, whether or not the totality of ^{the} scientific community decided to adopt it and then it's very jitteriness self-conscious. But it's the ~~thing~~ with which those things are approached -- that ~~is~~ is the point I'm trying to make, a norm that's not that ~~is~~ it's/always followed. I think that many historians are sufficiently ignorant about genetics for example that they will have made Lysenk^{ist}~~ist~~ types of attributions about the influence of some earlier environment on the hereditary in the ancestry of one of their subjects-

HZ Or excessively strong genetic interpretations.

JL Exactly. But I put that down to sheer ignorance.

JL Yes, at the very least they'd be cautious in doing so and
 it wasn't
 feel that/their job to try to try to measure the state of
 scientific belief on their data. That they have a hard
 enough job reconstructing from very fragmentary evidence
 any

RKM Could we go back to ^{revolution}~~revelation~~ now, once more?
 Without need to recapitulate it all, one general formulation
 is, what are the varieties of scientific revolutions
 and what does this case illumine with regard to the character
 of radical cognitive change in a branch of science or a
 field of science? And what are the models in terms of which
 one could examine this and which of those seem to work out.
 Now, is there anything else that is so to say the core of
 the paper? Is that the paper?

JL many things. The paper is a case study looking
 for

The Kuhnian issues and the Mertonian ones, there are the
 questions of the institutional arrangements as they influence
 the development of the work. I don't know whether Harriet
 has brought you up to date on this but, ^{we talked about} the role of the
 Army and Navy special training programs in
 keeping going a cadre of college students who were training
 during the war in college without which I might
 have preempted Henry Kissinger who managed to get drafted out
 of ^{School} ~~the~~ into the Army ~~corps~~, so that's what sort of brought that
 to the surface, because he's had a parallel individual

history He got in the army and I was in the Navy and that made all the difference in our outcome.

RKM

HAZ By the way I checked, Josh, in that volume, and it does look as though it was entirely possible for him to have been drafted and then to have been identified for the Army program. There were numerous ways people could get into it and that was at least one of them.

JL Well the Navy was strictly voluntary

But there's another point about that particular decision and that is that Columbia had a contract with the Navy not with the Army and one way of ensuring the continuity of my education at Columbia was the Naval route. Now whether I knew that at the time I enlisted or whether it could have been known, I don't know. I think that was pure luck that *the Navy was at Columbia*

HZ Well that turns out not to have been a matter of luck because
the requirements that the V12 program imposed on the universities
were so much more attractive than the Army program that good
universities would have found the Navy program
much more acceptable than the Army one. For example,

The V12 program permitted students to spend their first two years acquiring a liberal arts training.

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JL It was much less structured than the ASTP was. The ASTP
- the curriculum was specified by the Army

RKM thinking about institutional
arrangements, I'm trying to think of properties or characteristics
of those arrangements that may have been perceived and taken
into account in preferring one to another, just as ^ascientist
might say I prefer to be funded by ONR rather than
some of the competing sources of funds. Because the ONR
gives me much more scope. So here, the university administrator
might say, we prefer ^{the} V12 program. And the point on the
institutional arrangements is to ~~xxxxxx~~ treat the
fortuitous, historically fortuitous elements so that they
become institutional constraints or facilitators or channelers
of what it is we're really examining. That's what the
subset of issues we ^{are} ^{ing} deal_x with.

One raises the question, if you had gone elsewhere would
you have gone on to graduate work? By implication you're
saying this made it more likely.

JL Facilitation has been the key word throughout this discussion
Bob.

HZ That's a Mertonian theme which is not one which our ^{case studies} friends who do
address themselves to but which I think should be explicated.
Because otherwise I think it looks as if there were so many
chance occurrences, you can't explain anything.

JL Well, I don't recall that there was much more
 I think both of us will be doing some analysis of other
 materials we got down in the checklist of themes.
 I don't think this is the place to go/into the question of
 maturity and discontinuity in science

On the other hand the intellectual history will have to be
 an account that brings in discontinuity as well as ^{the} practical details
~~on bacterial~~ the sexuality. Make of it what you will, but I have a little
 trouble generalizing about that at this point. On the other
 hand the issues about macrocontinuity, that is the Kuhnian
 kind of thing, I think very naturally
 to this area. That is not to say that we both quite understand
 what we are going to say about it, but

HZ Josh, I don't think we've gotten awfully clear on the issue of
 who it was and why there was resistance to your work.

JL Well I've emphasized that in my dictation. Lwoff was
 the significant holdout. Lwoff made a fuss at the symposium,
 which was quieted quite promptly and I thought-one of the things
 I did dictate might not be too difficult to get some documentation
 on - diffusion and acceptance.

HZ But in neither case, just so I understand it in the Kuhnian
 framework, was the resistance because they were committed to
 bacteria not having sexual reproduction. It really had to do
 with a whole series of other--

JL No, they both had a specific counter proposal which came out of Lwoff's own experience in bacterial physiology

There was general reluctance, not to discount a long established tradition, I don't think many of them thought much about weakened experimental foundations were

sexuality not giving up lightly.

The fact that I came from Tatum's lab made me slightly more credible but-- the fact is that it took a year or two before people even wanted the cultures, they took what I said at face value in terms of experimental observation. Delbrück was just fanatically pig-headed about the kind of analysis that was being made and it showed in his own work on recombination and phage. He didn't want to believe it when he was doing it himself - in that particular context. And he does think in a very different style. Kinetics, kinetics is the ~~xxxx~~ only way he understands anything. If you describe the rate at which a process occurs and fit a mathematical model to it, then you understand it, otherwise not. So this is part of his rejection of chemistry, of material physics as opposed to biophysics or mathematical physics and so on. And he just flatly said - until the genetics has been worked out, demonstrated by such and such I see no reason to believe it or even take an interest in it.

- RKM Let's discourse for a time on the differences in style between you and Delbrück.
- JL Well he was someone who believed in deep mathematical models. He was on the track of a parallel to complementarity in biology. The only thing that was in fact a revolutionary finding that had to postulate something as dramatically different as indeterminacy in the biological sphere. His approach to it was one that lay very great emphasis on mathematical theory and very little on chemical observation and analysis.
- RKM When did he conclude that he understood something if I can use that expression. When he was able to literally write the equations?
- JL I think so. When he could write an equation that described the rate of the process, variations and variables.
- RKM How do you experience that?
- JL Well I accept that as a formal definition that at the point where I know all the things that I know how to learn by should be able to do what he's describing. But I'm willing to settle -
- RKM You understand him in the sense that you've been using the term understand.
- JL Well, I think so. I don't have his facility on the mathematical side.

RKM

JL But it's part of it. If I can't actually copy his equations or criticize them, he may be able to assert nonsense and I will not be able to [?]realize it. Not just but nonsense, so my criterion of understanding is that I can tell the difference between sense and nonsense. And I mean more than just solving a complex equation. It goes beyond that - the degree of abstractness

RKM Not any old equation. How would you visualize -

JL How well do you understand particle physics, Bob?

RKM Not at all.

JL OK. I'm not in a very different situation.

RKM Well, that's a strong statement.

HZ You mean it's that distant.

JL Yes.

RKM And it's not a matter of ~~xxxix~~ technique?

JL No, not at all. It's further than that. He wanted something that went beyond present visions of particle physics as explanations of biology.

HZ Remember that piece he did at the beginning of the Festschrift (PATOMB) that was reprinted from the Connecticut Academy, that speaks of deep paradoxes? But that must mean then Josh that ~~xxx~~ other people who are after all associated with his school, such as ~~Hirsch~~, ^{Hershey didn't follow that line.} His work has a different style and is much closer to yours--

JL Indeed, there's no difference between ^{Hershey}~~Hirsch~~ and mine. We're talking now about his philosophical position and how he operated on personal belief--

HZ But what did he say about ^(Delbrück) ~~Hx Hirsch~~ ^{Hershey}'s work? That's what I'm--

JL Well, as long as Hershey was willing to cowtow to him in the administration of the church and that's a little different from his feeling thoroughly sympathetic with the details of the way it was done. In fact it was Hershey and not Delbrück who did the labeling experiments with Delbrück would never in his life have done that experiment and in fact never did in his life. What Delbrück did at a time that our conceptions of bacterial phage were generally very very vague was to use that mathematical and then some notions of particles - in this case not deeply paradoxical ones at all - and very successfully used the Connecticut approach to understand them as particles better. But then when it came to their detailed interaction, when you had to give them a life of their own and deal with each phage particle ^{organismically} then

was fine and He went much further than I ever did in the level of abstraction in his models and it was inflexibility in trying to push them in their abstract form everywhere and he was disinterested in anything else. I think that disinterest was in a way not as hostile as many people may have believed but since it had

RKM Isn't there a tendency of most - not only physicists, high powered physicists to have something approaching contempt for what they're not interested in?

JL I recognize that breed. And they may be the people who ~~be~~ feel that they're the closest to the secrets of matter and

could compete very well as a theoretician in every other way and had none of those hangups.

The ~~phage~~ phage school and but that article by Fleming on the four phycists does rather well

RKM Do we have on the record any statement about your style?that's at all comparable to whatyou've been saying about this. I think it would be very helpful. Just try to characterize it. Would you hesitate to talk about your ~~own~~ own style?

JL I don't have as well defined a style. I'm much more eclectic.

RKM That's what I want to hear. So either you give it to us to use or you get it in but I think it's too important to omit.

What gives you a sense of deep understanding when you...

JL I don't really have it.

RKM What gives you a sense of accomplishment?

JL Well I think I did give you a piece of paper that I had written about twenty years ago about* the style of discovery that if I had a set of resources I'd be looking to see what could be done with them, efficacy being paramount and I really just become interested in the phenomenon and decide by God I want to find out how this particular thing works. Sometimes
start out with a methodological insight -

RKM And how a thing works is answered in what terms?

JL Well, reproducing it, no it goes beyond that, it means building a model but usually not a mathematical model but rather a mechanistic one. A clock has a gear in it and there's something else there and the general features of clocks, that they have got some way to tell the time and so, I don't want to make it too complete, I try to generalize from some essence of what I see there. But it's basically a pretty mechanistic sort of approach. And then I sort of flagellate myself ~~xxxxxxx~~ to go ahead and ^{say} let's try to deal with this in terms of a mathematical problem, try to pull those techniques in but they don't come that naturally.

RKM And conversely, when do you have a sense of problem, when do you have a sense there's something there to look into, what would be some of the episodes -

When do you invent a problem or come upon a problem?

JL Well I think there is a disparity between the resources we have and the ignorance we ^{have} /that ought to be cleared up. And then there are other efficacy judgements about ~~this about~~ the importance of this, that or the other

RKM Could you just list a series of such episodes where you made judgements Identify that this is worth looking into. This is not only worth looking into, this is what I am going to look into, so that there is at least a fighting chance of characterizing it. Now ~~we're~~ we're back to the enumeration problem, ~~we're~~

JL Well, ok. Let's go back to '45. DNA does something interesting to bacteria, the Avery paper, plainly touching upon something of vast importance in biology but vague, not clearly formulated, not a sharp, logical confrontation. That's ~~we're~~ another element. I would say that sort of semantic logical rather ~~xxxx~~ than mathematical analysis would be my forte there. John Platt talked about in his paper and then this kind of matching up of resources vs. aims. What can we do about this? And in that case there was - well one thing we can do, we have this organism which maybe Avery ^{really} doesn't/understand or know very well. It's got a beautiful clear cut genetics. Anything we do in ~~neurospora~~ ^{neurospora} is automatically accepted as relevant to the genetics of higher organisms able to understand it very well. Why don't we substitute a bacterium for a pneumococcus in Avery's equation and see what would happen

The other point about the r use
 we can/selective
 Because
 methods. ~~xxxxxxx~~/we have these biochemical ~~xxxxxx~~ mutants, we
 can control the environment that they're put into. We can decide
 whether or not it will grow or not grow. So it has a manipulability
 - experimental control which is greater than that in the other
 circumstance a cleaner experiment. You can confront a
 lot of r with a lot of DNA and make a qualitative
 prediction about what might happen, what the sensitivity of our
 assay is and be able to set boundaries ~~xx~~ ^{to}/what it is we are
 able to detect. Well, that thread you'll find yesterday and
 40 years ago. Of knowing the power of your method and being able
 to say in advance what its sensitivity is and what it's able to
 pick up. Then you do some experiments and you sort of see what
 happens and they may suggest/ ^{either} more methodological improvements
 or you may get a positive ~~xxxxx~~ answer that puts you on the trail
 of doing something interesting or you run into some blind alleys
 that make you go back and say ok, this part didn't work, what other
 permutations of the issue as originally presented have presented
 themselves during the over this question? So while
 worrying about how to put DNA I was
 certainly thinking consciously or unconsciously other manipulations
 any
 that might be possible or/other methods that might be applied. So
 up comes the idea of crossing in bacteria and then there's a
 branchover to *sexuality* . Is it so or isn't it so.
 In trying to lay the base for it, it's never been clearly tested so
 to
 let's think some more about how/do that experiment and then think
 through another experimental design with the same sensitivity,
 consideration, how do you assay for the result that you're trying
 can
 to look for, what method ~~xxxx~~ develop?

to look for, what method can/^{you}develop that will answer the issue
 when you've already got[†]a material appreciation that you've
 got enough resources to really go ahead and do something significant.
 There's a finite margin Then you go back to
 the lab and you fiddle for awhile and that's about the essence
 of that particular process. For another 10 or 15 years after
 that I was avalanched by data of various kinds. I just kept
 discovering one mechanism after another while using these tools
 and then sharpening the tools at the same time.
 a very powerful apparatus for exploring territory that had been
 inaccessible so it's a little hard there to dissect the different
 strands of decision about what to go into. Well the next major
 league, we'll talk about a minor one first--one^{cf} the things that
 Bernie Davis and I ~~did~~^{had} a multiple on was the penicillin method
 for the isolation of bacterial mutants and that was based upon
 a very superficial sort of impression about how penicillin worked,
 if it would make bacterial ~~lyse~~^{lyse} if they were growing and we'd
 been looking for methods by which we could remove cells able to
 grow and the paradox, we had to find the cells that couldn't grow
 in a given medium. It was easy to find the cells that could grow
 against the background of those that couldn't, but let's invert
 that and we'd be able to get our mutants much more readily. But
 we'd do the opposite of selecting for protot~~ops~~^{ops} out of the
somewhere about penicillin themselves
 only under conditions of growth and sure enough it works out that
 way that the nongrowing mutants remain dormant in a synthetic
 medium that doesn't let them grow, but if you plunk penicillin in

the ones that grow are the ones that are killed so the ones that were ~~xxx~~ asleep are the ones that stay alive and that's selective method.

And that's been worth a few hundred million dollars in the fermentation industry but its origin was the permutation of terms and the notion of how do you go about ~~this~~ systematic selection and at the methodological level. It wasn't such a crucial problem that needed to be solved at that time

and though by thinking of myself as a bacteri^{um}, "how does the world look to me?" I used that analogy with Harriet, that's that organismic insight. It's almost a mechanistic one. If I think I'm a clock I can figure out how ~~to~~ turn my wheels around. You can see ways in which you can encounter information about your environment and transform that to some useful output which then becomes a method. This became then another issue when a man called ~~discovered~~ discovered that the ~~lyses~~ ^{lyses} of bacteria with ~~of~~ of Fleming's first antibiotic, ~~first~~ first enzyme, could be prevented by putting the bacteria into high concentrated media called 10% sucrose solution. To do that, that would prevent them from ~~lysing~~ ^{lysing}. What happens there is ~~xxxx~~ the ~~lyses~~ ^{lyses} are cell walls of the bacteria and in an ordinary medium they'll just explode. But if you have a very highly concentrated medium the water is kept from diffusing into the cells and they're able to stay alive. So this was literally a Saturday afternoon experiment and I just asked myself, is it possible that penicillin ~~lyses~~ ^{lyses} through some similar method. It would

work just like that and then thinking about that we had some implications for the mechanism of action and so on. About how it worked as an antibiotic it would have to be an agent that specifically interferes with the synthesis of bacterial cell walls. if Therefore/the cells aren't growing— You don't need to synthesize them at all nothing much remains static but if the cells are growing and the walls are not being made to keep up with it they'll burst unless you protect them from bursting ~~with~~ by using a sucrose. So that that has become moderately important. There is a field of research on ~~the~~ spheroplasts - didn't like my calling them protoplasts that comes from that. It was a major insight to have that antibiotic work but what do you learn about style from that recitation?

RKM Well, what I will have learned about it, I'll learn after I go over that marvelous recitation.

JL All right but then let me go to a major league because I think that the first major departure that I can recall-- I took a sabbatical in '57, even before that around '53, '54, '55, I began worrying about, - now that we've developed this enormous methodology for dealing with the genetics of bacterial cells, what about other unicellular organisms like cells in tissue culture? Culture methods just beginning to be developed and Koch among others are starting to learn how to find appropriate media for them and

so I do another permutation and in this case strictly speculative, I've no intention of working on it myself, but try^{ing}/to develop a new field of s cell genetics. There again some erudition, going back in the literature and finding a lot of things that could fairly readily be done and never been tried out. There were even some hints that cell fusion might occur spontaneously. That there is sex in s cells an an analog of sex in bacteria. At several symposia, I ^{would} / just make speeches about it's time to start s cell genetics. And that you're not going to learn - a phrase that loved to quote from me more than anything else - is that if you want to learn about embryology you must eventually study an embryo. And you can't do it for all of the time on bacteria and on yeast which provided the/models at that point. But I was saying let's think of, let's work on embryos as if they were bacteria rather than rely solely on information about bacteria developing verified models without s cells. Well in fact in those early papers I pointed out the effect, among a list of things, that virus infection played some role in inducing cell fusion. It was not singled out. I didn't do any experiments of my own on it, but within the next few years, of course, Frissy started going on trying to do some genetics on s cells, and he said and Henry Harris showed in fact you could use a particular virus very effectively as a way of inducing fusion in such cells. And that's become a major discipline of its own. In a way it's pure

because these were strictly speculative ideas without going into a major investment in in trying to do those experiments. You may ask why and the why was that I had so much investment in bacteria that it didn't seem to be efficient to throw that away and go into a new field but it was efficient for me to simply point out those opportunities and let others--

HZ You were persuaded that what you were going to do with bacteria was just ^{as} interesting and just as important if one were to gauge it from the standpoint of the development of biology.

JL Yes, I thought molecular genetics were going to be solved with bacteria and I was struggling to try to get away in a sense from that entrancement by/sexuality into what that original question was about--the function of DNA. And in fact when I moved to Stanford it was in order ^{to} put aside the organismic genetics of ^E ~~the~~ coli and ~~to~~ try to get into the chemical study of DNA transfer. That was already '59. Wisconsin was not that congenial an environment for molecular genetics. Arthur Kronberg was moving here. It seemed like ^{the} ~~an~~ ideal place to get next to. And certainly from the point of view of those kinds of resources, it was, in order to pursue that kind of work. I have infinitely more competition in that arena ~~than I would in any arena that~~ today than I would in any arena that I went into in ~~the~~ the things that I'm interested in.

so I can't say it's been anything

like of outcome. But I continue to plug away there and I think it was not an unwise choice to make that particular shift. Then about s cell genetics, in '57 I took a Fulbright, about the only sabbatical I've ever had I guess and took about 4 months in Australia. I wanted to learn about virus recombination with Burnett. Burnett had published some confusing articles about influenza virus recombination and I knew he knew nothing about genetic analysis and I thought if I could use the same general approach that I had worked out in the on the phenomenon that he had discovered that influenza virus also showed recombination, we'd be able to make some headway. But when I got there I found he was no longer working on this, that he had started to work on the mechanism of antibody formation and so I worked with in his lab at that time and is now his successor as director of Institute, in looking at s cells do immune cells produce one antibody or more than one antibody, looking at it from a genetical perspective and in the framework of a selective theory of antibody formation. So that's the only work ~~of~~ I've ever done with my own hands on s cells-

RKM You keep coming back to the theme of what we've agreed to call by the code word, erudition. *There comes a time when* everything you've done reaches a certain point and then you go searching ~~for~~ the literature. Would it be worthwhile to focus on that as part of your style for a moment? To ask whether we can identify anything

in that activity that's all distinctive? Have we said everything by saying that you do a bibliographic search like everyone else? Do you do it in the same way as others?

JL No, not everybody else does it, in fact almost nobody else does it, so -

RKM OK. That's what I mean by differentiating.

JL Well, I don't know how to rationalize that-

RKM Well then describe it.

JL No I was going to make a hypothesis. I've been wondering why this almost neurotic insistence on looking for the antecedents of where I'm at at this point. And it has something to do with the fact that my education was in a library. I use the phrase on this account, that the library was my university throughout grade school, junior high* school, to a large extent high school and then in college ~~it~~ got all mixed up with what I was actually going to learn from others. But it was my main route of learning in such a systematic fashion, that it's a tool for me in which I have long self-indoctrination. ~~I was~~ very different from most of the students I run into these days.

HZ And you were after all indoctrinated into it at a time when you weren't in a hurry.

JL I had nothing else to do. I mean it was the major route of learning. I was just very deeply impressed by how much there was to learn and

I would get more- There's still a certain *dilemma* in terms of how I spend my time. Should I read more about something else, about some new subject. Or to what extent does that fact detract from some ~~thing~~ new external discovery. My own edification has been one of the imperatives and I've had the luxury of being able to do it from enough payoff on the other things I do.

HZ There was one line that Josh and I were developing one of the days you were in Berkeley which I think is worthwhile to reintroduce here since I think it would be congenial to Bob, ~~which~~ ^{which} is that since his work was done when he was so very young- (end of second side) he never had to go through the period of uncertainty in his career. He'd arrived just about the time he started.
beginning of 3rd side:

JL Well I was going to make the opposite comment. This is something that just occurred to me now, although it's a followup on what Harriet was just quoting. I leap-frog right across the conventional career structure. I was only formally enrolled as a graduate student after I had completed my thesis work. And jumped right through all the patterns of striving for recognition and ~~climbing~~ ^{climbing} up the ladder and so on. Once I had published a paper like that I could have sat ^{out} ~~the~~ the rest of my life and the fact is that I would have still had a position and all the rest of it. Though it would have been unsatisfying in other ways. But a very large part of the social pressures with respect to many norms of behavior of productivity and all the rest of it were not operative as external events. Some of us internalize ^{them} and we ~~xxx~~ ^{already} know how complex that was, how driven one can be just by that, maybe even more so.

But they never had that strong an external reality. Nobody said- you've got to do this, you've got to do that, or something very desirable of an external kind won't happen. And to a certain degree that's been true all of my life. Just during the period when there would have been the most socialization with respect to disciplining ^{ing my} ~~your~~ time, work effectively, if ~~you~~ ^I don't get that out in such and such a time ~~you won't~~ ^{I wouldn't} get your assistant professorship and so forth, I never had to experience that.

RKM I hazard a guess that that's more frequent than one would suppose. That some, not your extreme example, which makes it all the more interesting and so clear, but in a way I was fumbling toward that with regard to Tom Kuhn. If you looked at his publication pattern, it's the slowest, most long delayed you can imagine. which has to do with his own --

HZ But you don't know how he feels about it.

RKM No, that's what I'm saying, but it's what one would want to know and I don't know if he'll talk about it. But I'm looking at the external side of what you were describing, and I'd say, from the record as far as I know it, there was very little external pressure on Tom because he was sending out signals, not equivalent to your paper, which was decisive, that ^{his associates,} his mentors could have confidence in. And they relaxed the requirements, and they didn't say, "get it out, Tom." As far as I can guess. all that information I would want to get, corresponding to what we're getting here from you. But my conception there is, that in certain

university environments, despite the generic pressure imposed benevolently by the senior people upon their youngsters: you need to validate my judgement, you need to confirm it, the outside world needs to know that you exist - all the rest of it, that if we had good comparative data, top youngsters in top universities, are not subject to that in the same way as the other combinations--the average ones who need to put out to have any ~~chance~~ chance of making their way.

JL Well the appreciation of _____ can take the place of other objective output to some degree. I've had some of my own students where that was the case certainly, but only so far.

HZ And for so long. And I think it may be that the tolerance level would have been briefer in biology than it ^{had been} ~~would be~~ for Tom Kuhn.

JL Well in a way, Kuhn's sanctuary was his teaching rather than his research role. He could have remained for a very long time in that teaching function, a position which supported his self-esteem and so on in many other ways, at least to a considerable degree. He undoubtedly was encouraged/^{and} ~~by~~ the probability of moving in the direction of _____ research outlet was facilitated - to go back to our previous discussion - by some of the external incentives. They weren't negative pressures as much as positive ones. The Guggenheim and the Center thing and so on, because I'm sure that he perceived that it was not for his teaching that he was going to ^{those} be given/~~by~~ carrots even if they were being offered on the promise rather than the performance. Well, you're raising what is inherently

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51

a statistical question. I think there would be some interest in pursuing that. To put it a little differently, it has a certain bearing on the kinds of issues that you're raising in your article on ageing and so forth, on age parameters as they appear on scientific careers--

RKM Of course, intersecting that institutional context, organizational context and the whole debate on publish or perish not only in the popular arena doesn't formulate the interesting questions. For which subset is it the case for which it is publish or perish? How can you then find the occasions where it is certainly not publish that is involved but it is ability which can take a variety of forms.

JL Yes, but the forms are rare. ** You have to work three times as hard in other ways to make up for publication which we all know to our distress would turn that evaluation around. A few lousy papers go a long way. Well, I think this question of age and so on is something/not to ~~xx~~ probably be gone into in this article but I think for the book there are some interesting angles there. Let me say a few more things though about a research style and we're talking about problem choice as well as/going into them. The next big jump in my research interests - there were ~~and~~ digressions like starting in the department of medical genetics, to medical teaching, sort of working within the discipline as an institution, trying to broaden ~~x~~ it and get for it the recognition and the impact I thought it deserved and so forth, which was

JL Yes - you ~~know~~ need a biochemist to teach biochemistry to medical students. If you have a geneticist teaching medical students, it's going to have some bearing on the definition of the discipline. I conceivably might have been thought of by ^{his} as a candidate for/~~the~~ department. But I guess I would have been a pretty marginal biochemist. But medicine seemed to/^{me}~~be~~ the arena in which there would ~~be~~ most likely be ~~any~~ applications. And you know enough of the rest of my background to know that *molecular biology* was not suddenly invented in the mid 50s but it was coming home again. Wisconsin was not the most attractive place to be ^{medical} trying to do it. It's a good second-rate/ school but it was still better than your strict agricultural school.

JL It's a unanimous judgement shared undoubtedly by the people who
are there too. It caters to the requirements of the state of
Wisconsin. It has rather strict residency requirements
and so forth.

So the chance to come to a ~~x~~ place like Stanford ~~was~~ with this tremendous vigor

It was pretty obvious even then that Stanford was going to become a major university. It was just on the verge of turning that way. It was quite irresistible. But again that's not a research interest. Although it starts draining off time and energy in thinking about a lot of extra-scientific issues. The next big jump research-wise though was ~~exobiology~~. And there, there was part of this very broad range of concern and interest and you know the fundamental questions of the origin of life, is there life on other planets but not focused on a very operational outcome but of course Sputnik turned that over over night. So here was the research tool-

here now we have a research tool we didn't have before. There are things we can do with it that open up new opportunities for the understanding of some very fundamental questions. The question of - is there life on other ~~planets~~ planets is no longer a theoretical speculation. You don't need much erudition, or I had whatever was necessary long since to know that that was an unresolved issue by terrestrial observation, that it could become an operational question. And that pragmatic note is an element of style. Things become really interesting when you can ~~know~~ can do something about it being quite bold and saying what that thing is. It isn't the theoretical ~~possibility~~^{im}/probability of the idea but the possibility of a tangible^{ible} experiment that one can do, which is the main criterion. If something has not been

are
 proven to be false you/still ~~have~~ entitled to think about it as
 if ~~it~~ it might be true. It it doesn't violate ~~the~~ laws of physics
 and chemistry and so on. So that was also tied into a political
 perspective which *Haldane* reinforced when I got to see him.
 I don't know if I've told you any of this story or not. But anyhow
 I have ~~it~~ it written down someplace else for another novel.
 But this was near the end of my stay in Melbourne, that Sputnik
 arrived. I think it was the 6th of October 1957. We saw it in
 Melbourne essentially the day it was launched. It had a southern
 hemisphere lighting trajectory and sure enough there it was. I
 must have seen it on the second or third revolution and the whole
 world thought about it as you very well remember in very different
 ways. But I was already thinking about it as maybe there was
 something we could do about it to enlarge the scope of biology.
 But not with any serious intention of getting into it myself, just
 - isn't this interesting now, maybe my grandchildren will have
 the opportunity to see this materialize sort of thing. I'd arranged
 to return home via India at *Haldane's* invitation - he said stop
 over and see us in Calcutta and did so. That turned out to be
 the date of a lunar eclipse and I by these calendars
 it was also the anniversary of the October revolution although
 it was ~~x~~ early November and we were speculating - the Russians
 are going to pull another one now - they'll use the occasion to
 light up a big red star on the moon that ~~w~~ll be visible ~~xx~~ forever.

Very seriously, somewhat metaphorically but the *Haldane speculation* was made seriously - you know - taunting me with that the communist system really does pay off in the long run. The mobilization of effort and so on and they're really showing your friends, the Americans. You may recall he had only recently emigrated from England with the statement that he chose no longer to live in an American colony. So we stayed up late that night to see if it would really happen but we also did some back of the envelope kind of calculations on what would be required to make a spot that might be visible from the earth and we concluded that a large hydrogen bomb explosion might just barely ~~make~~ ^{make} it. And looking carefully with a large telescope you might just manage to see it in that fashion and we didn't seriously think they would do that with all the other implications. We sort of shrugged our shoulders and started ~~■~~ discussing some further implications of it but what was in that discussion was that the whole enterprise was going to become a political demonstration not a scientific effort. And that's why I became sufficiently anxious ^{to decide} /to jump into it myself, I just didn't think many of my fellow scientists were going to be quick enough to appreciate ~~that~~ the significance of this event and that that particular thing needed to be forestalled. That there needed to be an impetus to that program to keep it from becoming merely a political demonstration, particularly if you want to do things like introduce a lot of radioactivity on to the moon or contaminate the moon with bugs and other things that might carelessly destroy their scientific ^{utility} / - apart from diverting them, might irrevocably destroy them. So that's how

I started a campaign ~~on~~ on how to get the National Academy organized ~~and~~ into worrying about ~~planetary~~ planetary quarantine and to setting up a scientific program for the exploration of space and so forth. This was before NASA itself was in fact ~~not~~ organized and so there was enough response to that so that NASA did get to be organized and they've been organized ever since and gradually got pulled into doing something constructive they would challenge me - they would say - look you've been an effective critic and it's been very helpful but don't you want to do something positive in this direction? When I came out to Stanford I somewhat reluctantly/set up an engineering lab ~~to~~ -- Does that tell you about style?

RKM Yes it does. So obvious on the surface is first your ~~xx~~ recurrent pattern of becoming interested for whatever reason, in the area of phenomena or events or potentialities but then ~~xx~~ ripening very swiftly once you encounter a tool, a possibility of doing something about it. For looking into it empirically, looking into it in a developmental way. So your sputnik and ~~diagnoses~~ ^{Neurospora} are functional equivalents.

HZ But is that a really accurate portrayal^{al} of the sequence? Because I had the impression from Josh that it sometimes happened that he really didn't have a pattern of interest but that when he saw that there was a procedure available - ^{he} ~~you~~ then searched for a problem to attach it to.

JL Yes but then when an issue materializes, I guess I have to say that's right, I ~~can~~ can organize a very *effective* effort and in a pretty short time make things happen. I tried to pick out the things where that was possible.

HZ And I underscore that, Bob, because at least in our field, there's a prevailing view that there's something intellectually unrespectable about being interested in procedures for themselves and I think it would be worthwhile to - *emphasize this.*

RKM Well if you could just dictate a list sometime as swiftly as it comes to mind - of what's called - whatever your favorite term is - the tools -

JL When you get back to NY why don't you get this part of the tape transcribed?

Well I think that kind of eclectic, pragmatic ~~kind of~~ translation oscillating back and forth is highly opportunistic. And it's driven by external events to a very large degree, but I don't blush about that. That's how I find the most efficient and resources available ~~at a given time~~ at a given time. I don't have any great ideology about what the problems are

I think each generation ~~has~~ discovers new ones and that my own role is to work on those things where I can make an impact. I am always ^{that} aware, there's an efficiency criterion.

Why should I muddle around with the stuff that everyone else is doing? I remember telling me that when I first started on *molecular* biology. He said - Josh why don't you get out of molecular biology now - you've already made your impact on it, you'll be one among many and you're wasting your time relatively there. ~~xxxxxxxx~~ Nobody well I never quite did that but

RKM I just want this on the record - whatever else - whenever else - I want a crack at the style of scientific work

has done - I want to find out what can be formulated in a more general way about what is generically recognized as an interesting question. What are the varieties, ways in which different kinds of scientific output come ~~into being~~ or the variety of ways in which the same kind - you look at outcomes - and I suspect that the ^{Edge-}~~ex-~~ *Mulkay* would be the purely descriptive sort of thing that turn out in biographies of scientists how *Darwin* or Pasteur or ~~others~~ others go about their work

JL The criteria are not often articulated.

RKM That's what I mean by having a crack at it. That is so intimately connected with the--

JL Well it's become somewhat self-conscious Bob. That is I spent some time trying to discover what my style was - I showed you my note on that - and at this point I actually use that methodology as a part of my own pattern of discovery about what to do- It's coming to a head right now. I've been rather concerned about

the directions my own lab ought to take for a variety of reasons. And some of the organizational activity you heard about is connected with it and for the moment - just among ourselves - my plan now is to gradually retire from the kind of molecular genetics I've been doing in favor of Stan Cohen. If he comes into the department, ^{he} can provide that kind of intellectual leadership, I could really be more efficient by occasionally advising him than I can by trying to maintain my own program in that area. And vice versa, one of the main reasons I wanted to stay in that game over the past 6 or 8 years has not been that I felt that I could make a distinctive contribution within it but when I started in DNA splicing I was the only one who really thought much of the idea but I wanted to have the test bed on which to examine the ^(?) heuristic methodology of science in connection with the descendants of the Dendral Project and without having a working laboratory in which we're basing these kinds of issues every day I didn't feel we could offer enough input that research in ^(?) heuristics of scientific advance and putting it on the computer that's the other side of the applied social science we've been talking about. About how to get science better done. Getting Stan into the dept solved many problems simultaneously that way, but he can do a better job than I can in terms of the very vital program in that area. He's cooperative and interested in computers as well but he's not nearly as distracted as I am in about 16 other things and a very bright person and he'll do a better job.

So I'm sort of pondering what can I still do in that arena that follows the stipulations that I just indicated and what can I do that many others can't afford to do for one reason or another and so on, And partly as part of the retrospections we talked about here, partly a theme I've returned to briefly about once every 10 years is I think we're going to look for the missing links in terrestrial life and they've got to be there some place. All the organisms we know now have DNA, RNA, 20 amino acids fully developed, the genetic code in full blossom and they must have of stages in their evolution and the prevailing doctrine and there's no evidence to the contrary is that seem to be by the further evolution of life but can we find some living fossils of an earlier stage of evolutionary development working and we're ~~graping~~ for more efficient methods by which that notion could be attacked so here you see (selective mechanism - HZ) - that's right, some selective procedures or some thought about what habitats they might still be in, so this is kind of bringing Mars back to earth if you like. And you can see its connection with that tradition. But there the emphasis you see is on the problem not on the method, I don't have a method now and I probably won't go into it seriously without having some insight into some new approach to getting after ~~it~~ it - but for instance - about 15 years ago I had the idea that maybe there were missing links that had only RNA and not DNA and I started setting up experiments to see if there was a way of finding organisms that had only RNA. And one thing that had occurred to me was to

use flotation methods that's ide method to
 capture organisms with an unusually high density. I didn't
 actually get to do that. I had three or four other approaches
 which didn't pan out, that was not a high priority kind of
 issue in any way at all so I didn't follow it up. But I have
 some reinforcement from the fact that ^{Norton} ~~Maxxxx~~ Zinder had a
 rather similar thought and used a systematic methodology in
 fact to discover RNA phages. He was the first to find that
 there were phages that ~~xxxx~~ had RNA and not DNA

HZ Are phages in the small category of living fossils or not?

JL No, because they're not free living you see they can only multiply
 within other cells. They ~~xxxxxx~~ presumably are eddeys of ~~xx~~
 the complete ~~x~~ cycle DNA RNA protein. And other RNA viruses
 have been known for a long time so it's just the fact that these
 were bacteriophage RNA that could be isolated by selective
 methodology oriented to what you were looking for. We don't really
 know where they come from but one is permitted to assume that and
 you can't go any further. The only conditions under which they
 proliferate are as parasites in a large organism.

RKM Well whether you find yourself focusing on a problem first or on
 a method, procedure or tool how would you describe, fairly concretely,
 your attitude of mind when you consider - what can I do-

JL Opportunism is more important than the focus on method or problem.
 The method provides the opportunity and so the opportunism can go
 to work immediately. Here ^{the} ~~ten~~ opportunity is the fact that it is

an unpopular problem. It's a bold one and it has many of the attributes ^{that} ~~about~~ sexuality in bacteria ^{had} ~~/~~. It really makes better sense in terms of the overall evolutionary continuity of life on earth if such organisms can still be around but merely because nobody has noticed them so far, there's a prevailing assumption, ~~there's~~ a myth of hopelessness about it but nobody's really looked and there are still a lot of organisms in the existing catalogue that have never been assayed for what's in them. And there are ^{variant} ~~xxxxxxx~~ questions related to it. Why does every organism have just the same 20 amino acids. Why didn't one innovate a little bit and substitute amino acid x for existing amino acid y? Well there are some feeble arguments at this point and I am not very much convinced by them. So opportunity in this case ~~is just~~ ^{of} consists ~~in~~ separating the myth from the verified and science ~~is~~ full of such things but ~~■~~ these are some of the more important questions-

RKM Incidentally before I forget I think you should write Roger Hahn. Did he send you a set of the papers that ~~xxxx~~ already were ^{put together} ~~in the~~ ^{Proceedings} ~~1~~ ? Get the supplementary papers, particularly the one by ^{Hywell} ~~White~~ (I forget his name) and Dan Sullivan.

HZ It's White, Sullivan and Barboni.

RKM It's Barboni, White, Sullivan. And read ^{that} ~~xxxx~~ in terms of their problem, the relationship between the theorists, what they call the phenomenologists, and what they call the experimenters in physics. And they have developed a very nice simple procedure for

trying to gauge the degree of interaction between these types in the course of *developing a specialty*. but as I was listening to you this afternoon it was reverberating. A second thing to look for, I don't know, I'll send you the more exact reference or location of one of the papers I scanned, I don't think it's in the original volume, but it's one of the other papers, raises many questions, It may be the Edge paper for that matter - about the notion of resistance in science - that it's a conceptually for the most part - resistance - as Bernie Barber identified it - is a conceptual misconception-that what's seen as resistance or simply alternative perceptions and conceptions - at any rate, it's a useful ~~xxx~~ thing to have ~~xxx~~ them raise that question, corresponding to reexamining one's own -

JL Has anyone pointed out that you are under obligation to resist? That even if you are persuaded by an argument that you ought to put on the role of not believing it?

RKM Yes, that's organized skepticism. Except you put it in a form that you might be able to - in fact we can use - that's what we should do - when we get into really hot water in this effort to formulate the norms in this national survey - in forms that will avoid responses in ideological terms which are meaningless and in irrelevant terms. To find something that's cuts close to the bone of every day attitudes and such.

JL I had *thought this was* a little bit different organized skepticism. I thought that a ~~u~~ skeptical attitude was inculcated and that ~~it~~^u *there*

was reinforcement^e for being nasty and hostile.

RKM No, for doubting, systematic doubt-

JL I'm talking about the posture of doubt. One should apply this to oneself. One knows that it's very difficult to do that and after a while you become persuaded by your own arguments and therefore you should expose it to others, who don't have those attached commitments and are able to adopt that posture without as much internal conflict, and have ended that position that they may in fact ~~have~~ preferred.-- But why do people write about Mirsky the way they do and pan him, every commentator about the Mirsky-Avery relationship makes a demon out of him and a fool.

HZ Did Avery perceive him as a demon? I should think not.

JL It's very hard to say. Avery never expressed himself.

HZ But in the papers he is very careful to give the Mirsky line its due.

RKM Is that the way they perceived him back then - perceived Mirsky.

JL Well I guess my perception is a little different from others because I was imbued with this imperative of skepticism somewhat more deeply than some of my colleagues and I agreed with Mirsky longer than most. In fact I sometimes articulated very similar positions. So I'm not a fair test on that point.

RKM
~~SE~~

No, I didn't mean you, I meant the attacks on Mirsky - when do they occur? Are they retrospective attacks.

JL Mostly retrospective.

RKM So it's a it's an interpretation.

JL Exactly. But all the as careful to avoid that
as I ~~am~~ would like. Only the fact that he was wrong ~~xxxxxxxxxxxx~~
is totally irrelevant. It seemed a little irritating at a point
when everybody wanted to believe that the philosopher's stone had
in fact been crystallized, this person was raising what ~~is~~ still
are
to my view/~~x~~ factually correct criticisms. And Martin Zinger and
Mac McCarty (?)
said yes I guess we have to agree, number is
and that is a very large number. That's the scope of
how many molecules of impurity you could sneak into a preparation
that was 99.999% pure and they would there's
no way we can make this stuff any purer. And then Mirsky and
I would pop up ~~and~~ - yes we understand that but that .001% could
still have that information we're talking about and we need some
other evidence. And they ~~do~~ would come back ~~what~~ operationally-
what do you want us to do about it that would settle it? And ~~then~~
they were in a little bit of a dilemma because we were not able to
suggest other experiments on it.

RKM Did you assume that the critic should be in a position to propose
the needed experiment, to show its feasibility or withhold

JL No. But it would have been better if one could I think. The
critic who really criticizes and doesn't make more constructive
suggestions is being a little bit irresponsible, at least after
the ~~the~~ ^{entire} time or so

RKM So that's a normative evaluation whether it's shared or not.

JL Well things hadn't really quite gotten to that point. The DNA could be was See one of the problems is ~~was~~ that pneumococcus assay is very crude and therefore for quantitative measurements of the amount of the magic material were rather poor. I would have been content if they could have shown that reached an in terms of the specific activity of your material, that further purifications dedicated to removing the last traces of protein no longer even changed the relevant activity. And I didn't feel that their methods of assay were good enough to be able to make that statement. There were other fussy experiments about mixing things together and removing the protein again, which were more or less dedicated to the same thing. You could add miscellaneous protein to the system, remove it again, hope that that would remove at least a portion of the magic protein in question, and then you should have a lower specific activity.

HZ Does this protein have an attractive quality for its like kind?

JL It should, you expect one protein to be able to displace another one that was not an equal part of the molecule. Eventually we had other approaches. had completely homogeneous DNA populations as we have today, those questions are far less cogent than they were at that time. fractionate that DNA molecule that was doing something from a different DNA molecule and show that the DNA was different. I guess in a sense that was first done in

